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Interactive comment on "Increasing turbidity in the North Sea during the 20th century due to changing wave climate" *by* Robert J. Wilson and Michael R. Heath

Anonymous Referee #3

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General Comments: The authors investigate the relationship between suspended particulate matter (SPM) and bed shear stress (BSS) by means of historic, satellite and model data. They motivate well in their literature review that decreasing water clarity in the North Sea may be linked to increased SPM content. The premise of this work is enticing. It can help to motivate further research and provide an explanation for the long term increase of water turbidity. I find the paper to be well written, language wise, and the motivation and analysis part to be comprehensive, but the analysis needs to be more quantitative. Particularly, I like the message that changing wave regimes should not be neglected in long term simulations with reference to climate change. There are some details and nit-picks that need reworking.

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Discussion paper

Introduction: The statement of SPM increase possibly exceeding 20% needs to be made cautiously. While the method of Hakanson 2006 is perhaps not ideal to show this, I am more concerned about the vague phrasing. I find no basis for it.

The authors mention several times that tides can be assumed to be free of long-term changes, which is not exactly true, pedantically speaking. There are long period tides (see e.g. Wunsch 1967), which may be negligible directly due to their low amplitude (<1cm) but they play a role in low frequency climate oscillations. Furthermore, sea level rise has an effect on the tidal regime. It is certainly more feasible to neglect them, but then perhaps this should be mentioned.

Methods: There is a vast amount of data used and it would be helpful to expand on the particular choices of data sources and organise it for the reader's eyes (perhaps in a table or figure). Some data was taken from CMEMS/MetO-NWS-REAN-PHYS to determine if a water column was stratified (section 2.1), but depth-averaged velocities were taken from an FVCOM model, while those same velocities are available at CMEMS as well. It would be helpful to motivate the individual data choices. There may have been easier choices for a unified data set with fewer independent sources.

Page 3, line 4 (diffusivity): as a physicist, I do understand the role of diffusivity, given that turbulent diffusivity is of course in orders similar to sinking velocities. So perhaps just add the word "turbulent" there. The threshold of 0.5°C appears arbitrary and needs further explaining. One could e.g. refer and compare to the definition of the mixed layer depth (MLD) used in CMEMS/MetO-NWS-REAN-PHYS or Kara et al. 2000. Alternatively, one could just use said CMEMS data of the MLD instead of coming up with a new one (i.e. if the MLD is smaller than the water depth, the column is stratified). Because the ERA-interim and ERA20c are different data sets, they cover a combined period of 1990-2017. It should be made clear that there is no combined data set or otherwise how a potential integration is carried out and bias is made impossible.

I am unfamiliar with R, but as far as I can see, no tremendously complex statistical operations have been carried out that would require elaboration beyond textbook knowledge and I would know how to achieve the same results in MATLAB. However, it is perhaps helpful to provide some algorithms as flow diagrams in a supplement.

Results: The results section starts off with an explanation of a seasonal climatology of near surface SPM, as well as BSS. This would be more suitable in the methods chapter. Figure 1 is the first of several cases where the authors say in the text that there was something to see in the figure which is actually hard to see (in this case the seasonal cycle of BSS, which is noted in the text but not well visible in the image).

For figure 2, the same criticism applies as for figure 1: the text says that there is a clear positive relationship, but the figure shows dark blue colours on the left panels in several areas where the right panels show bright yellow. It needs to be made clearer what constitutes as a "clear positive relationship", i.e. a by threshold value or something of the sort and the colour maps need to be modified accordingly. For example, in the text (page 7, line 4ff) it says that the transition to mixed water increases the link between SPM and BSS, which can be seen south west of Ireland in November, but in the figure, it is dark blue there, which indicates a weak correlation. The message that figure 2 carries could be made clearer also by an area correlation, which is more quantitative than a visual comparison.

The monthly stratification was not previously described as climatological, as it is presented here in figure 2. Instead it was written in section 2.1 that the percentage of stratified water columns was taken for each month over a period of 20y. Since the analysis covers 20y and the BSS is assumed to change due to changes in wind stress, the stratification would potentially show trends as well due to changed turbulent mixing. Climatological stratification thus makes less sense than monthly means over 20y, unless it can be shown or motivated that the change in stratification is negligible. In the first paragraph of page 7, it says that SPM and BSS become uncoupled in stratified regions in summer months. However, with reference to figure 1, the authors claim a seasonality in both parameters. What is the reason for the uncoupling? Can it be explained?

The description of the methodology for figure 3 belongs in the methods chapter (perhaps 2.4), not the figure caption, and it needs elaboration. It says “For each mapped box and month, grid points with a 20y record of SPM are selected.” Are all grid points within a box selected for which there are 20y of continuous data, or are these random choices? Were the grid points that do not have continuous data coverage neglected? Why was a complete area average unfeasible? Furthermore, the authors again say that there is a “clear positive relationship”, which is easy to see e.g. for box 12, but not so much for e.g. box 1. Regression parameters are in the supplemented tables, but it would be much handier if they were besides the respective plots as well (at least R2).

Also, all boxes are of the same size, but some are only partially covered with water, some cross widely different domains, physically speaking (e.g. box 11 covering parts of the Rhine and East Anglia plume, but reaching close to the Dogger Bank, box 5 covering the Norwegian trench and thus depths from 50-300m). This may be as a minor nit-pick, but it could be argued that a more appropriate choice of boxes could have been made (e.g. as in Capuzzo et al. 2015 or O’Driscall 2014/ICES boxes). In figure 4, the changes in Secchi depth are marked as blue and red, yet ranging from +50 to +50 to positive. Red is presumably negative, i.e. a decline, so it should be -50% there. I really struggle with the sentence p.9, l.5. The evidence indicates an increase, more than it indicates a decline or no change (in that the plotted points are blue, and strongly so). Unless the data coverage is sufficient to make a claim, a claim should not be made. Perhaps a measure of certainty should be given (e.g. through marker size).

Again for figure 4, the relationship between decline in Secchi depth and bed stress change is hard to see at first. This may be due to sparsity of data, and as the authors stated earlier, the SPM content in the areas south of 53°N are heavily influenced by river intrusions. Hence, modifying the map by highlighting areas of high river intrusions could help clarify the link between the left panels and the right. Furthermore, a less selective method of data comparison than choosing points with 50km of each other might help here as well.

At p.9, l.8, it says that there was a significant increase in BSS across the entire shelf between 1910-1929 and 1990-2019. This is immensely confusing, because previously (figure S2), a trend of decreasing BSS was shown for the latter period, so it invokes the understanding that the two periods are investigated individually, and not against each other. Perhaps this could be clarified by rephrasing. In the same paragraph it says that the changes are driven by increased significant wave height (SWH), which could be shown in a figure, e.g. by an area correlation. Is there literature as to why the increases in SWH were so variant over space?

Discussion: In the first paragraph of the discussion, it is argued that there is a decline in primary production (PP), which is attributed to reduced clarity. However, figure S1 shows decreasing trends in SPM. There is a need for elaboration as to why there can be declining SPM as a main contributor to turbidity and reduced PP. The authors later provide this elaboration on page 11, but for easier understanding, the two paragraphs should be interwoven. As a side note: a large number of Secchi depth measurements are taken from near shore stations, e.g. the NIOZ facility on Texel, NL. This will heavily skew measurements in a surrounding area.

The statement in line 13-14 is too strong and needs to either be more strongly motivated (quantitatively), or weakened. In line 19 on page 10, it is again said that there would be an expected increase of 20% in SPM. There needs to be a source for this claim and a direct reference as to how this claim can be made.

In lines 12ff on page 11, biological activity is mentioned as a potential impact on SPM and BSS. Note that before 1950, larger areas of the North Sea had benthic flora (see e.g. Capuzzo et al. 2015), which impacts BSS heavily (and thus also tides, as to my earlier point).

Minor comments: A search for typos and grammar mistakes is appropriate. My main points of criticism are with the figures, as stated above.

Page 6, line 1: “Fig. 2 shows the R2 value for the linear regressions of 8-day SPM and bed shear stress and SPM for each month between 1997 and 2017, and stratification throughout the year.” This sentence is hard to follow. Perhaps it should be “. . . the linear regressions of 8-day SPM and bed shear stress [. . .] for each month. . .”? Line 2: “The relationship between bed shear stress shows a seasonal switch.”, seems to be missing that extra “SPM” from line 2. Line 5: “. . . driven. . . ”.

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